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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Case study of a humidity layer above Arctic stratocumulus using balloon-borne turbulence and radiation measurements and large eddy simulations" *by* Ulrike Egerer et al.

Anonymous Referee #2

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Peer review of manuscript "Case study of a humidity layer above Arctic stratocumulus using balloon-borne turbulence and radiation measurements and large eddy simulations" by Egerer et al., for potential publication in ACP.

This paper uniquely combines in situ observations, turbulence theory and associated parameterizations, and LES modeling to explore the connection between cloud top processes and overlying humidity inversions in the Arctic. The authors do a great job describing the field campaign and the methodology used to analyze the measurements and modeling studies. From a relatively limited measurement period, it is found that

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SHIs are likely intimately connected to the cloud via turbulent kinetic energy production likely associated with cloud top radiative divergence and/or gravity wave fluctuations across the stable temperature inversion layer. These results confirm a number of observationally-based hypotheses and LES modeling studies, where it has often been speculated that SHIs play an important role in Arctic cloud physical characteristics and cloud lifetime.

I find the paper to be very well written. I am particularly delighted to see the combination of unique, state-of-the-art in situ measurements analyzed in complement with turbulence theory and LES modeling. I only have very minor concerns, which are outlined below. Once the authors address these concerns, I would be happy to support the publication of this manuscript in ACP.

General comments

1) I appreciate the discussion regarding the potential biasing of humidity inversions due to sensor wetting during the ascent through a cloud layer; this has been a caveat or concern in the community for some time, considering many of our climatological frequencies of SHI occurrences have been derived from radiosoundings from field campaigns. It is great to see the ascent/decent profiles of humidity from the BELUGA system do in fact show similar thermodynamic structures to the radio soundings.

Have any additional tests been made to attempt to isolate cases where the radiosounding-derived SHIs are potentially biased by sensor wetting, in which case these profiles could be removed from the analysis? I wonder if it would be helpful to broadly estimate the adiabatic liquid water content of the cloud layer from the thermodynamic profiles, and make a comparison with the absolute increase in specific humidity within the SHI (i.e., sensor wetting should likely not exceed the maximum LWC value in the profile). Surely the amount of sensor wetting must be limited by the maximum amount of cloud liquid water content(?).

2) The analysis and conclusions derived in this study come from really only 2 profile

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cases. And even these 2 case have substantial variability in the physical properties of the inversion structures, the flux magnitude estimates, and the turbulence characteristics. I am missing an attempt by the authors to characterize or relate the flux estimates (negative) to the properties of the temperature and humidity inversion layers. How might the displacement depth between SHI base/max and level of largest infrared divergence (cooling) affect the results? I would like to see some more of this substance in the discussion Section 5.

Specific comments

Line 26: See/include reference to Devasthale et al. (2011, ACP: "Characteristics of water-vapor inversions observed over the Arctic by Atmospheric Infrared Sounder (AIRS) and radiosondes")

Line 52. The section heading "Observational" is an adjective, and therefore requires a noun to follow. Please adjust accordingly.

Line 95. It seems to me, from Fig. 2, that the other two balloon flights during the 5-7th June also correspond with the 12 UTC sounding time and have a continuous ascent and descent profile. The authors should explain, or show, why the results from this soundings and balloon profiles are not shown or described in the text. Do the profile comparisons not look as convincing as in Fig. 1?

Line 100. It would be helpful to include the cloud boundaries from Cloudnet at the time of the balloon decent as well. This may help to explain the discrepancy between RH and cloud boundaries.

Line 114-115: I am confused. I thought the Cloudnet retrievals included ceilometer base heights, MWR liquid water path estimates, and thermodynamic profiles from soundings to retrieve cloud boundaries?

Line 124-125: It would be helpful to include the cloud base and top heights (as colored symbols) on the normalized profiles, in order to show whether (and how deep) the

cloud top extended into the temperature and humidity inversion structures.

Line 145-146: Note additional studies as references: Sedlar et al. (2012, JCLIM); Shupe et al. (2013, ACP); Sedlar and Shupe (2014, ACP); Brooks et al. (2017, JGR).

Line 157: Between which depths in the layer are the Ri number calculated?

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